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GEORGE BISHOP, Esq. Treasurer, in the Chair.

The following communications were read:—

I. Observations of Halley's Comet, made at the Observatory of Geneva in the years 1835 and 1836. By M. Müller, under the direction of M. Gautier, Director of the Observatory. Communicated by Sir J. F. W. Herschel, Bart.

These observations were made on fifty-two nights, beginning with August 31, 1835, and ending with May 7, 1836; of which thirty-one were before the perihelion passage of the comet, and twenty-one after the passage. The instrument used is an equatoreal of Gambey, whose telescope has an object-glass of four French inches diameter, and of forty-two French inches focal length. The declination circle and the hour circle of the instrument are each thirty inches in diameter; the former being divided to every three minutes of a degree, and by means of its verniers giving arcs of 3″; and the latter being divided in time, and by means of its verniers giving the fifth part of seconds of time. The times were taken with a clock by Lepaute, which was every evening compared with the transit clock. The index corrections, obtained chiefly by observations of stars found in the A. S. C., and whose observed places were compared with the places taken from that Catalogue, and from Pond's Catalogue of 1112 stars, were very consistent throughout the whole series of observations, and shew that the firmness of the instrument, as well as its state of adjustment, were highly satisfactory. Absolute observations of both elements were obtained in every instance by reading off both circles; this method being preferred by M. Gautier to differential observations with a micrometer. A reticular micrometer, made of fine plates of metal, was used, the faintness of the comet scarcely ever admitting of any illumination of the field.

In the reduction of the observations, the mean refractions were computed for all the observations of the comet and the comparison-stars; and the instrumental right ascensions and north polar dis-

tances are given, cleared of the effects of them. The index corrections obtained from all the observations of stars are also given. It is, however, left to those who may be desirous of using the observations of the comet to apply them, and also the effects of parallax, to the observed places.

The height of the observatory above the level of the sea (above 400 mètres) caused the comet to be visible at this observatory longer than at most other places in Europe; and the author hopes that this circumstance may render the latter part of the series especially valuable, the southern position of the comet and the unfavourable state of the weather causing the observations of it to be in general very scarce, after its perihelion passage.

II. Note on the Masses of *Venus* and *Mercury*. By R. W. Rothman, Esq.

If the longitudes of the ascending nodes of *Mercury*, *Venus*, *Mars*, *Jupiter*, *Saturn*, and *Uranus*, be respectively designated by Ω_0, Ω_1, Ω_2, Ω_3, Ω_4, Ω_5, Ω_6, and the inclinations of their orbits to the ecliptic by ι_0, ι_1, ι_2, ι_3, ι_4, ι_5, ι_6, then the motion of the node of *Venus* on the real ecliptic may, as is well known, be expressed by the equation:—

$$\begin{aligned}\frac{d\Omega_1}{dt} = &-\Big\{(1,0)+(1,2)+(1,3)+(1,4)+(1,5)+(1,6)\Big\}-(2,1)\\
&+\Big\{(1,0)-(2,0)\Big\}\frac{\tan\iota_0}{\tan\iota_1}\cdot\cos(\Omega_1-\Omega_0)\\
&+\Big\{(1,3)-(2,3)\Big\}\frac{\tan\iota_3}{\tan\iota_1}\cdot\cos(\Omega_1-\Omega_3)\\
&+\Big\{(1,4)-(2,4)\Big\}\frac{\tan\iota_4}{\tan\iota_1}\cdot\cos(\Omega_1-\Omega_4)\\
&+\Big\{(1,5)-(2,5)\Big\}\frac{\tan\iota_5}{\tan\iota_1}\cdot\cos(\Omega_1-\Omega_5)\\
&+\Big\{(1,6)-(2,6)\Big\}\frac{\tan\iota_6}{\tan\iota_1}\cdot\cos(\Omega_1-\Omega_6)\end{aligned}$$

where (1,0), (1,2) ..... (2,0), (2,1) ..... are certain quantities depending on the masses and major-axes.—*Mécan. Céleste*, Vol. I. p. 295. I have calculated these quantities with the following values, multiplying the masses assumed respectively by the factors $(1+\mu_0)$, $(1+\mu_1)$, $(1+\mu_2)$ ..... where μ_0, μ_1, μ_2, are undetermined corrections of the same.

	Masses Assumed.	Semiaxes Major.
☿	$\frac{1}{1909706}$	0·38709812
♀	$\frac{1}{401839}$	0·72333230
♁	$\frac{1}{356354}$	1·00000000
♂	$\frac{1}{2680337}$	1·52369352
⚳	$\frac{1}{1046·77}$	5·20116636
♃	$\frac{1}{3512}$	9·53787090
♅	$\frac{1}{20000}$	19·18330500

I have thus obtained

$$(1,0) = (1+\mu_0)\ 0''{\cdot}44799 \qquad (2,0) = (1+\mu_0)\ 0''{\cdot}10351$$
$$(1,2) = (1+\mu_2)\ 6{\cdot}86011 \qquad (2,1) = (1+\mu_1)\ 5{\cdot}17404$$
$$(1,3) = (1+\mu_3)\ 0{\cdot}10205 \qquad (2,3) = (1+\mu_3)\ 0{\cdot}29823$$
$$(1,4) = (1+\mu_4)\ 4{\cdot}21136 \qquad (2,4) = (1+\mu_4)\ 7{\cdot}08278$$
$$(1,5) = (1+\mu_5)\ 0{\cdot}19836 \qquad (2,5) = (1+\mu_5)\ 0{\cdot}32565$$
$$(1,6) = (1+\mu_6)\ 0{\cdot}00425 \qquad (2,6) = (1+\mu_6)\ 0{\cdot}00692$$

Taking then the following values of the nodes and inclinations for 1800 (which is nearly a mean epoch, for the interval I am about to consider), viz.:—

$$\Omega_0 = 45^\circ\ 57'\ 9'' \qquad i_0 = 7^\circ\ 0'\ 5''{\cdot}9$$
$$\Omega_1 = 74\ 51\ 41 \qquad i_1 = 3\ 23\ 28{\cdot}5$$
$$\Omega_2 = 0\ 0\ 0 \qquad i_2 = 0\ 0\ 0$$
$$\Omega_3 = 47\ 59\ 38 \qquad i_3 = 1\ 51\ 6{\cdot}2$$
$$\Omega_4 = 98\ 25\ 45 \qquad i_4 = 1\ 18\ 51{\cdot}6$$
$$\Omega_5 = 111\ 56\ 7 \qquad i_5 = 2\ 29\ 35{\cdot}9$$
$$\Omega_6 = 72\ 59\ 21 \qquad i_6 = 0\ 46\ 28{\cdot}0$$

I obtain, finally,

$$\frac{d\Omega_1}{dt} = -17''{\cdot}5632 \quad +0''{\cdot}1770\ \mu_0 - 5''{\cdot}1740\ \mu_1 - 6''{\cdot}8601\ \mu_2$$
$$-\ 0{\cdot}1975\ \mu_3 - 5{\cdot}2304\ \mu_4 - 0{\cdot}2730\ \mu_5 - 0{\cdot}0051\ \mu_6$$

Now, Mr. Glaisher's researches give the observed tropical motion of the node from 1750 to 1836 = +30″·95 annually, which, with a mean precession of 50″·22, leaves for the annual sidereal motion of the node −19″·27:—

$$\therefore\ -1''{\cdot}71 = 0''{\cdot}177\ \mu_0 - 5''{\cdot}174\ \mu_1 - 6''{\cdot}860\ \mu_2 - 0''{\cdot}198\ \mu_3$$
$$-5{\cdot}230\ \mu_4 - 0{\cdot}273\ \mu_5 - 0{\cdot}005\ \mu_6$$

And it follows, that if the masses of the other planets be rightly determined, the mass of *Venus* must be increased by about three-tenths.

To form an idea of the possible error of this determination, let us suppose the assumed mass of *Uranus* to be wrong by one-fourth, that of *Saturn* by one-tenth, that of *Jupiter* by one-hundredth, *Mars* one-fifth, the *Earth* one-hundredth, and *Mercury* one-fifth, which are liberal allowances. Let us also suppose, which is very improbable, that the signs of these errors are such as to affect the result with the sum of the whole. Finally, let us admit an uncertainty on $\frac{d\,\Omega_1}{dt}$, as found from observation, to the extent of $0''{\cdot}05$. Yet, with all these suppositions, μ_1 cannot be less than $\frac{1{\cdot}46}{5{\cdot}17} = 0{\cdot}28$. I may add, that the results here obtained appear to indicate that the mass of *Mercury* should be diminished; and this is confirmed by a consideration of the motion of the perihelion of *Venus*. This motion is expressed by the following equation, in which π_0, π_1, π_2, represent the perihelia of the respective planets.

$$\frac{d\pi_1}{dt} = \left\{(1,0)-(1,0)\frac{e_0}{e}\cos.\,(\pi_0-\pi_1)\right\}\{1+\mu_0\}$$
$$+\left\{(1,2)-(1,2)\frac{e_2}{e_1}\cos.\,(\pi_2-\pi_1)\right\}\{1+\mu_2\}$$
$$+\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$
$$\text{\&c.}\qquad\text{\&c.}\qquad\text{\&c.}$$

Reducing this expression to numbers with the same assumed masses as before, and taking the values of e_0, e_1, e_2, &c. π_0, π_1, π_2, &c. for 1800, I get

$$\frac{d\pi_1}{dt} = -2''{\cdot}4137-4''{\cdot}5753\,\mu_0$$

after supposing μ_2, μ_3, μ_4, μ_5, μ_6, each equal to zero. (It will be remarked that the mass of *Venus* does not enter into this expression.) Now, Mr. Glaisher has obtained for the annual tropical motion of the perihelion of *Venus* $49''{\cdot}62$, or, with a precession of $50''{\cdot}22$, a sidereal motion of $-0''{\cdot}60$:—

$$\therefore\ -0''{\cdot}60 = -2''{\cdot}41-4''{\cdot}58\,\mu_0$$
$$\text{and } \mu_0 = -0{\cdot}40 \text{ approximately.}$$

From the uncertainty attaching to the observed amount of the perihelion motion of *Venus*, I only give this result as confirming the idea that the mass of *Mercury* must be diminished.

On the whole, it is very remarkable that the planetary masses given in the *Mécanique Céleste* (Vol. III. p. 61), satisfy the secular motions affecting the orbit of *Venus* much better than the masses of later astronomers. It appears that in later times the mass of *Mercury* has been too much increased, and that of *Venus* too much diminished. What has been previously remarked concerning the masses of *Venus* and *Mercury* is confirmed by the motion of the

node of *Mercury*. If this motion be calculated by theory with the masses of the *Mécanique Céleste*, the result agrees almost exactly with the motion determined from observation by Lindenau. — See his *Tabulæ Mercurii*, p. 9.

III. Observations of the Immersion of p^2 *Leonis* behind the Dark Limb of the Moon. By R. Snow, Esq.

The observed Ashurst sidereal time of the immersion was $15^h\ 37^m\ 23^s{\cdot}9$. The observation was made with a power of 75 on the telescope of the five-feet equatoreal, under very favourable circumstances.

IV. Extract of a Letter from Professor Encke to Mr. Airy, dated 20th December, 1841. Translated from the German. Communicated by G. B. Airy, Esq.

The comet of short period comes to perihelion on the 12th of April next; and judging from its present course, and from former experience during Mr. Henderson's residence at the Cape of Good Hope, it may be well observed there during the end of April and the whole of May, and probably also in June. May I then trouble you with the request to get the accompanying ephemeris conveyed there, or to the southern hemisphere generally, and also to provide for its circulation in England? I should think that, with the present active communication between England and all parts of the world, there is yet time enough to send the ephemeris to the astronomer at the Cape of Good Hope by the end of March; and it would have the greater interest for him, because, to this time, *no* return of the comet since 1819 has been missed. The ephemeris is not strictly founded on all the earlier observations, because it was impossible for me, notwithstanding all my endeavours, to reduce completely the observations of the comet made here in 1838. The compared stars still required some more observations for their determination. Meanwhile I have provisionally determined a correction of the last observations of 1838, or rather a correction of the calculations relating to that time, which will not be far from the truth. Upon this provisional reduction, and the calculation of the perturbations produced by *Jupiter alone*, the elements now given for 1842 are founded. Judging, however, from earlier experience, I believe that even with this incomplete calculation, the predicted place will be wrong by only about a few minutes. The error certainly cannot amount to half a degree, consequently the comet must be found, if it is really visible, and if the search be made with care.

The difference between the observation and the calculation, in the year 1838, is with great probability to be attributed to a very important error in the hitherto received mass of *Mercury*. This mass is the same that Lagrange, in the *Berlin Memoirs* 1782, has derived from a hypothesis on the density of the planets, according to which the density ought to increase very much with their proximity to the sun. Laplace has adopted the number without alteration. In

order to obtain approximately the mass of *Mercury*, which has *never* yet been determined, I have first substituted the most accurate values of the other masses. Thanks to your excellent observations, which are completely confirmed by Bessel (Bessel finds the mass of *Jupiter* $= \frac{1}{1047 \cdot 871}$), we are quite clear about the most important of them, that of *Jupiter*. The others have a smaller influence, even if they are erroneous. I have then used a double calculation. In the first I have assumed that till 1835 inclusive, the earlier determined elements were quite accurate. In fact, the errors of 1832 and 1835 are small. With this assumption I have sought to remove the apparent error in 1838 by an alteration of the mass of *Mercury* and an alteration of the constant of the resistance. This succeeded perfectly, and I obtained a slightly changed constant of the resistance and a mass of *Mercury* $= \frac{1}{3091947}$, about $\frac{2}{3}$ of the former mass. In the second calculation I have endeavoured to remove the errors of 1832 and 1835 as well as that of 1838 (each being previously treated as if it had arisen from an erroneous time of perihelion passage), by an alteration of the same elements; namely, the mass of *Mercury*, and the constant of the resistance. Here remain likewise only small differences, if the constant of the resistance is left quite unchanged, and the mass of *Mercury* assumed at $\frac{1}{4865751}$, or about $\frac{5}{12}$ of that of Lagrange. This last determination I intend to adopt as a basis for accurate comparison, and to correct it more closely. It has the truly remarkable peculiarity that with it the *densities* are,—

☿ = 1·12	♀ = 0·92	♁ = 1·00	♂ = 0·95
☉ = 0·25	♃ = 0·24	♄ = 0·14	♅ = 0·24

Consequently, the solar system appears to be composed of two sets of bodies, whose densities are either nearly = 1 or = $\frac{1}{4}$, and which are separated by the interval between *Mars* and *Jupiter*, where there exists no large planet, but only the four small ones.

Ephemeris of the Periodical (Encke's) Comet, 1842.

ELEMENTS.

Epoch, 1842, April 12·0^h Berlin mean time.
Mean anomaly ..... 359° 58′ 34″·3
Sidereal daily motion ..... 1070″·61433
Angle of eccentricity ..... φ = 57° 39′ 13″·8 (e = sin φ)
Longitude of perihelion ..... 157 30 4 ·7 } Mean Equinox
Longitude of ascending node ..... 334 39 1 ·8 } of April 12, 1842.
Inclination ..... 13 20 24 ·8

The Right Ascensions and Declinations are referred to the Mean Equinox of April 12, 1842.

1842. 0^h Berlin Mean Time.	Right Ascension of Comet		Declination of Comet.	Comet's Log. Distance	
	In Arc.	In Time.		from the Earth.	from the Sun.
	° ′ ″	h m s	° ″ ′		
Jan. 26	351 30 5	23 26 0·3	+ 4 46 57	0·29172	
28	352 13 3	23 28 52·2	5 3 20	0·29020	
30	352 57 19	23 31 49·3	5 20 21	0·28847	0·15938
Feb. 1	353 42 55	23 34 51·7	5 38 0	0·28652	
3	354 29 53	23 37 59·5	5 56 17	0·28433	
5	355 18 14	23 41 12·9	6 15 13	0·28192	
7	356 8 1	23 44 32·1	6 34 47	0·27925	0·12431
9	356 59 15	23 47 57·0	6 54 59	0·27634	
11	357 51 59	23 51 27·9	7 15 49	0·27316	
13	358 46 15	23 55 5·0	7 37 16	0·26971	
15	359 42 6	23 58 48·4	7 59 21	0·26598	0·08421
17	0 39 36	0 2 38·4	8 22 3	0·26196	
19	1 38 47	0 6 35·1	8 45 22	0·25762	
21	2 39 44	0 10 38·9	9 9 17	0·25297	
23	3 42 32	0 14 50·1	9 33 47	0·24799	0·03759
25	4 47 17	0 19 9·1	9 58 51	0·24265	
27	5 54 3	0 23 36·2	10 24 29	0·23694	
March 1	7 2 57	0 28 11·8	10 50 39	0·23084	
3	8 14 6	0 32 56·4	11 17 20	0·22433	9·98237
5	9 27 37	0 37 50·5	11 44 29	0·21736	
7	10 43 39	0 42 54·6	12 12 4	0·20992	
9	12 2 18	0 48 9·2	12 39 59	0·20197	
11	13 23 44	0 53 34·9	13 8 11	0·19345	9·91550
13	14 48 4	0 59 12·3	13 36 32	0·18433	
15	16 15 28	1 5 1·9	14 4 54	0·17454	
17	17 46 2	1 11 4·1	14 33 5	0·16401	
19	19 19 51	1 17 19·4	15 0 50	0·15267	9·83250
21	20 56 58	1 23 47·9	15 27 51	0·14041	
23	22 37 20	1 30 29·3	15 53 40	0·12713	
25	24 20 48	1 37 23·2	16 17 46	0·11268	
27	26 7 0	1 44 28·0	16 39 22	0·09691	9·72841
29	27 55 11	1 51 40·7	16 57 28	0·07962	
31	29 44 9	1 58 56·6	17 10 46	0·06061	
April 2	31 31 54	2 6 7·6	17 17 28	0·03964	
4	33 15 24	2 13 1·6	17 15 17	0·01650	9·60828
6	34 50 12	2 19 20·8	17 1 20	9·99102	
8	36 10 22	2 24 41·5	16 32 19	9·96325	
10	37 8 46	2 28 35·1	15 44 48	9·93347	
12	37 38 22	2 30 33·5	14 36 1	9·90241	9·53775
14	37 33 57	2 30 15·8	13 4 45	9·87118	
16	36 53 55	2 27 35·7	+11 12 0	9·84115	

1842. 0h Berlin Mean Time.		Right Ascension of Comet		Declination of Comet.	Comet's Log. Distance	
		In Arc.	In Time.		from the Earth.	from the Sun.
		° ′ ″	h m s	° ′ ″		
April	18	35 40 44	2 22 42·9	+ 9 1 3	9·81365	
	20	34 0 5	2 16 0·3	6 36 53	9·78968	9·60596
	22	31 59 10	2 7 56·7	+ 4 5 15	9·76985	
	24	29 45 28	1 59 1·9	— 1 31 50	9·75428	
	26	27 25 26	1 49 41·7	0 58 32	9·74276	
	28	25 4 15	1 40 17·0	3 22 15	9·73482	9·72610
	30	22 45 37	1 31 2·5	5 37 0	9·72987	
May	2	20 31 57	1 22 7·8	7 41 41	9·72732	
	4	18 24 36	1 13 38·4	9 36 5	9·72658	
	6	16 24 5	1 5 36·3	11 20 40	9·72716	9·83064
	8	14 30 21	0 58 1·4	12 56 16	9·72866	
	10	12 43 0	0 50 52·0	14 23 53	9·73074	
	12	11 1 20	0 44 5·3	15 44 38	9·73316	
	14	9 24 36	0 37 38·4	16 59 32	9·73574	9·91400
	16	7 51 55	0 31 27·7	18 9 34	9·73834	
	18	6 22 28	0 25 29·9	19 15 36	9·74088	
	20	4 55 23	0 19 41·5	20 18 25	9·74329	
	22	3 29 53	0 13 59·6	21 18 39	9·74554	9·98115
	24	2 5 12	0 8 20·8	22 16 53	9·74763	
	26	0 40 35	0 2 42·3	23 13 34	9·74954	
	28	359 15 21	23 57 1·4	24 9 6	9·75130	
	30	357 48 51	23 51 15·4	25 3 48	9·75292	0·03658
June	1	356 20 27	23 45 21·8	25 57 53	9·75446	
	3	354 49 34	23 39 18·3	26 51 30	9·75595	
	5	353 15 40	23 33 2·7	27 44 42	9·75743	
	7	351 38 16	23 26 33·1	28 37 31	9·75897	0·08338
	9	349 56 57	23 19 47·8	29 29 52	9·76064	
	11	348 11 24	23 12 45·6	30 21 36	9·76248	
	13	346 21 21	23 5 25·4	31 12 29	9·76458	
	15	344 26 39	22 57 46·6	32 2 15	9·76698	0·12357
	17	342 27 16	22 49 49·1	32 50 36	9·76976	
	19	340 23 17	22 41 33·1	33 37 10	9·77298	
	21	338 14 54	22 32 59·6	34 21 35	9·77668	
	23	336 2 26	22 24 9·7	35 3 29	9·78092	0·15871
	25	333 46 18	22 15 5·2	35 42 28	9·78574	
	27	331 27 4	22 5 48·3	36 18 12	9·79117	
	29	329 5 24	21 56 21·6	36 50 22	9·79724	
July	1	326 42 7	21 46 48·5	37 18 43	9·80397	0·18978
	3	324 18 5	21 37 12·3	37 43 1	9·81136	
	5	321 54 12	21 27 36·8	38 3 9	9·81942	
	7	319 31 28	21 18 5·9	38 19 3	9·82812	
	9	317 10 50	21 8 43·3	38 30 46	9·83745	0·21756
	11	314 53 15	20 59 33·0	38 38 24	9·84738	
	13	312 39 31	20 50 38·1	38 42 9	9·85786	
	15	310 30 26	20 42 1·7	38 42 14	9·86886	
	17	308 26 37	20 33 46·5	38 38 59	9·88032	0·24259
	19	306 28 33	20 25 54·2	38 32 43	9·89220	
	21	304 36 35	20 18 26·3	38 23 47	9·90443	
	23	302 51 0	20 11 24·0	38 12 32	9·91697	
	25	301 11 54	20 4 47·6	37 59 18	9·92977	0·26541
	27	299 39 21	19 58 37·4	37 44 24	9·94278	
	29	298 13 17	19 52 53·1	—37 28 9	9·95596	

V. Comparisons of the Planet *Venus* in Right Ascension and N. P. D. with the Star A. S. C. 423, made with the Equatoreal Instrument of the Observatory at Ashurst, on April 9, 1841. By R. Snow, Esq.

The equatoreal instrument employed for these observations is of Fraunhofer's construction, and furnished with clockwork; the object-glass is of five feet focal length, and of four inches aperture. It is supported on a very firm pier, and retains its position very well.

The observations were made with a position micrometer, adjusted for transit and declination observations. They consist of thirty transits of the star and of the first limb of *Venus* over the meridian wire, and of nine micrometrical measures of the differences of N. P. D. of the star and the south limb of the planet: the corrected sidereal times of the observations are given.

The value of a revolution of the micrometer-screw had been determined by 400 transits of stars near the equator. Measures of the semi-diameter of *Venus* were made at the same time, by which it was found that the measured value exceeded the tabular value given in the *Nautical Almanac* by $3''{\cdot}1$.

The circumstances of the observations were favourable.

VI. Reduction of Mr. Snow's Observations of *Venus* and the Star A. S. C. 423, with some remarks upon the employment of equatoreals in Planetary Observations. By the Rev. Richard Sheepshanks.

Mr. Snow's observations admitted of being so grouped as to furnish four sets of comparisons in right ascension and five sets in declination. The effects of parallax and refraction were computed by the formulæ used at Greenwich (*Greenwich Observations*, 1836, pages lxiv. and lxv.) The right ascension of the star was taken from Lord Wrottesley's Catalogue, the declination from the A. S. Catalogue, and the semidiameter of *Venus* from Mr. Snow's Observations; and thus the right ascension and declination of the planet were obtained for the Ashurst sidereal times of observation and compared with the places interpolated from the *Nautical Almanac* for the same times. The resulting corrections to be applied to the right ascensions and declinations of the *Nautical Almanac* are as follow:

Right Ascension.	Declination.
s	″
− 1·10 from 15 obs.	+ 3·1 from 1 obs.
− 1·32 ... 5	+ 5·4 ... 1
− 1·25 ... 5	+ 5·3 ... 4
− 1·27 ... 5	+ 5·2 ... 2
	+ 3·6 ... 1
Mean − 1·19 from 30 obs.	Mean + 4·9 from 9 obs.

The mean epoch is about $8^h\ 30^m$ Greenwich mean solar time.

The author remarks generally with respect to the treatment of such observations, that they may be boldly grouped without sensible

error, so as to make one reduction serve for a considerable number of observations; and that to ensure the greatest facility for grouping, the observations of one element (if both cannot be made simultaneously) should be repeated several times as rapidly as possible alternately with similar sets of observations of the other element.

With respect to the value of such observations, the results above given will shew that an equatoreal, when thus used, is no mean rival to meridian instruments. The star can be subsequently determined with any required degree of accuracy, and the observations can be made with as great freedom from constant error with an equatoreal as in the meridian. In this latter respect, indeed, the power of repetition gives to the equatoreal a great superiority, and may be made to counterbalance the disadvantages arising from want of steadiness. The last-named quality can, however, in most instances, be obtained in as great a degree as is requisite. The hour-circle being firmly clamped, if the instrument be well balanced, sudden changes can arise only from careless handling.

The supposed uncertainty and instability of the adjustments are probably the principal obstacle to the free use of equatoreals in England; but the author considers that most equatoreals can be adjusted very nearly, and that when ordinary care has been taken, the position remains sufficiently permanent; and it is certain that when rationally used, the effect of any unavoidable derangement is so nearly annihilated as to be quite insensible. The *difficulty* of performing the adjustments of an equatoreal is very trifling, if it be methodically undertaken, and the residual errors much smaller than would at first sight seem possible. With well-turned collars and pivots an error of half a minute, arising from flexure or other causes, must be looked upon as an impossible quantity, in which case the differential effects upon objects in the zodiac might be disregarded. With respect to methods of observing, the author recommends that the telescope be moved in declination like a transit, in order that the star and planet may pass over the same part of the wire. In this case reliance is placed only on the adjustment of the cross-axis; but when the declination is not changed, it is presumed that the position of the wire is correct; and this can be ascertained with only a moderate degree of certainty. In equatoreals which can be reversed in every position, the observations should be made, one group in one position, and the second in the position reversed. The best wiring for such observations, the author considers to consist of three, five, or seven immovable wires, at equal distances, and parallel to the meridian, transit-wires, in fact, and seven equidistant wires at right angles to these, at 5′ interval, the plate which carries the latter wires being moved by a micrometer-screw. The advantages of this system are a saving of time in screwing the micrometer, less wear of the screw, and less dependence on it for large intervals.

Thus far it has been shewn, that an equatoreal instrument may be made to rival meridian instruments, by the bestowal of a little more time and trouble; there are, however, many cases where the equa-

toreal is more convenient, and many where *it* can, and the *others* cannot be used.

A planet which comes to the meridian at a late or inconvenient hour of the night may be observed several hours earlier with the equatoreal. In so variable a climate as ours, it is not too much to say, that the number of good planetary observations might be thus very much increased; and if an equatoreal were steadily directed to this object in the southern hemisphere, to meet the case where the planet has considerable south declination, we should soon have the materials by which the present sufficiency of theory might be satisfactorily tested. The superior planets cannot always be observed in full daylight with large meridian instruments, yet equatoreals of even a small size might be made to determine their places with great accuracy after sunset. Again, large equatoreals, which are now tolerably abundant, might take charge of the minor planets. Micrometrical observations only have been taken notice of in the preceding remarks, the divided circles of the instrument being considered only as finders, and for performing the adjustment, though in some instruments they are large and good enough to be used in differential observations. Still the proper use of the equatoreal is the ascertaining of small differences by means of the micrometer and time.

In conducting the observations, the author recommends that there should be made each night two or three transits of the star of comparison, and of two other stars, one above and one below it a few degrees, the instrument being clamped in right ascension, by which means it would be made evident whether the derangement of the adjustment had any sensible effect upon the place of the planet. It is the want of observations to *accuse* derangement which makes the stars observed as moon-culminators less satisfactory than if they were more widely spread in declination.

With respect to observations of the moon, the author mentions one set, originally suggested by Struve, but never carried into effect. There are three observations which might be made when a bright star is occulted by or reappears from under the moon's bright limb:—

1. The time of disappearance or reappearance of the star.
2. Micrometrical measures of distance between the star and the moon's bright limb, the clock-work and the wire micrometer with the slipping piece being used.

(This is the common observation of distance, and might be usefully applied to the case of a near approach.)

3. Differences of right ascension between the moon and star, the hour-circle being clamped as in ordinary transit observations.

If the place of the moon be computed from these three observations, we *ought* to arrive at the same result; and if we do not, the difference between the first and second result arises from the moon's irradiation, and will give a measure of it; also a difference between the second and third results would shew some error in the mode of

taking the transit of the moon's limb, which is at present rather a doubtful point in practical astronomy. If by certain corrections, constant either to the observer or the telescope, these results can be made to agree in *each* case, and *always* the longitude might be determined in a shorter period, though with more calculation than at present, and a greater certainty be obtained from transits of the moon's limb.

In conclusion, the author hopes that the attention of persons who possess good equatoreals may be directed to the planets whenever those bodies are favourably situated with respect to an observable star. The adjustment is really nothing, and if pairs of stars above and below be observed, any error arising from mal-adjustment can be ascertained and allowed for. The artist will take care, if warned, that the cross-axis shall be at right angles to the polar axis, and the reductions, in ordinary cases, are very trifling, especially if by judicious grouping one reduction is made to serve for several observations.

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MONTHLY NOTICES

OF THE

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Admiral MANNERS, President, in the Chair.

W. Lloyd Wise, Esq., Greenhythe,
was balloted for and duly elected a Fellow of the Society.

On the Rejection of Discordant Observations.
By E. J. Stone, Esq.

In the discussion of the results of observation there is nothing more unsatisfactory than the necessity, which occasionally arises, for the rejection of certain results because of their discordance with the general average given by numerous observations. These discordant results will in many cases be found to be deduced from observations made by observers of great experience and skill, and apparently under most favourable circumstances. In such cases our minds are almost irresistibly led to assume that causes of error must have been in action other than those causes which lead to the ordinary graduated errors. It is clear, however, that such assumption of irregular causes must not be made without great caution and reserve. So far as I know only two criteria for the rejection of apparently anomalous results have been published. In No. 45 of Dr. Gould's *Astronomical Journal*, Professor Peirce has published an investigation leading to formulæ of some complexity for the rejection of such observations. The employment of these formulæ is rather troublesome, and grave doubts have been expressed with respect to the accuracy of the

heory upon which they are based. See Airy, No. 90 of Gould's *Astronomical Journal.*

In Chauvenet's most valuable *Treatise on Astronomy*, vol. ii. page 565, will be found a simple criterion for the rejection of one observation. This criterion is introduced ostensibly for the purpose of clearing up certain difficulties in the understanding of Peirce's criterion. Chauvenet's criterion may be stated as follows: —

If $\theta(x)$ denote the probable number of errors equal to or greater than x, when the whole number of observations is denoted by unity, then the number of errors equal to or greater than x, which may fairly be expected in n observations is $n\,\theta(x)$. If therefore we find x such that $n\,\theta(x) = \frac{1}{2}$, any error greater than x will have a greater probability against it than for it, and may, therefore, be rejected.

This criterion appears to me to be based on an erroneous principle. In $2n$ such observations we ought reasonably to expect one error equal to or larger than x. In n observations therefore we ought not to be surprised at the appearance of such an error, and certainly its appearance would be no fair ground for an assumption of some disturbing cause of error.

I have been led, on consideration of this subject, to a formula which embodies, in my opinion, the true grounds upon which my judgment rests when compelled to reject discordant observations.

If a is the ordinary probability parameter of a class of observations, we have for the probability of an error equal to or greater than p,

$$\frac{2}{\sqrt{\pi}} \int_{p}^{\infty} \epsilon^{-\frac{x^2}{a^2}} \frac{dx}{a},$$

or if $y = \frac{x}{a}$, the probability

$$= \frac{2}{\sqrt{\pi}} \int_{\frac{p}{a}}^{\infty} \epsilon^{-y^2}\, dy.$$

As a matter of fact, we know that *mistakes* do occur in the making and registering of observations. To confine our attention, for example, to observations in zenith distance, the observer may omit to clamp the circle and the instrument may be moved between the bisection of a star and the reading of the microscopes. He may clamp the circle inadvertently, after having made his bisection. He may bring the declination wire nearly to bisect the star, with the intention of completing the bisection at the centre of the field, and then forget to make the bisection. He may accidentally touch the declination screw, or micrometer heads, after the bisections have been made and before the read-

ings have been registered. He may read off wrongly the heads of his micrometers or transpose his readings in entering them in the observing book; or he may enter erroneously the readings of the barometers or thermometers required for the calculation of the refraction corrections. These, and possibly other, errors we know that an experienced and careful observer may make in entire ignorance of there being anything wrong in his work. We have here a series of causes of errors entirely distinct from those which lead to the ordinary graduated errors.

For a given class of observations and for a given observer there must exist a number n, which expresses the average number of observations which that person makes with one mistake. n may be defined as the modulus of carelessness.

If, therefore, we find that value of p, which makes

$$\frac{2}{\sqrt{\pi}} \int_{\frac{p}{a}}^{\infty} \epsilon^{-y^2} \, dy = \frac{1}{n},$$

all larger values of p are, with greater probability, to be attributed to mistakes than to the ordinary run of graduated errors. Larger values of error than p must therefore be rejected. If e denote the probable error of an observation, we have

$$e = 0{\cdot}4769 \times a,$$

our equation for the determination of p becomes therefore

$$\frac{2}{\sqrt{\pi}} \int_{\frac{0{\cdot}4769}{e}}^{\infty} \epsilon^{-y^2} \, dy = \frac{1}{n}.$$

From which, so long as n remains constant, we see that the limit of rejection varies directly as the ordinary probable error.

The following table gives the limits of rejection, with a probable error $0''{\cdot}477$, for different values of the modulus of carelessness n. It may be mentioned that the assumed probable error is about that of a Greenwich observation of *Polaris* in N.P.D.

Value of n.	Limit of Rejection.
	$''$
5	0·95
10	1·2
20	1·4
40	1·6
80	1·8
100	1·9
1000	2·3
10000	2·7

If this theory is correct, the meaning of these numbers is as follows. Suppose we are satisfied that on an average our observer makes one mistake in a thousand observations. I mean such a mistake as cannot be detected and corrected with perfect, or almost perfect certainty. Then, with a probable error of $0''{\cdot}477$, all discordances, from the mean of a great number of observations, greater than $2''{\cdot}35$, should be at once rejected.

Discordances so large as $2''{\cdot}75$ should not be retained unless we are prepared to believe that our observer only makes one mistake in making and registering 10,000 observations. Discordances so small as $0''{\cdot}95$ ought not to be rejected unless we are prepared to believe that, on an average, our observer makes one mistake in five observations. It is perhaps impossible to determine the absolute value of n for any class of observations, but an inspection of the tables shows that the limit of rejection varies very slowly for any values of n which would be admissible in the case of observations made by careful and experienced persons. A rough value of n is quite sufficient for practical purposes, but when once a value of n is admitted, all rejections are determinate. If this theory is correct our rejection cannot be made, except upon a direct admission of carelessness.

The following table would give, according to this theory, the limits of rejection for different probable errors with an assumed value of $n = 500$:—

Probable Error.	Limit of Rejection.
″	″
0·12	0·55
0·48	2·2
0·95	4·4
1·91	8·8
3·82	17·6

or very roughly about five times the probable error.

Note on the Employment of the Table given in the Nautical Almanac, "showing the Correction required on account of Second Differences in finding the Greenwich Time corresponding to a reduced Lunar Distance." By Capt. Shadwell, R.N.

Finding the time corresponding to a reduced lunar distance is effected by the inverse application of the general formula of interpolation

$$F_{(n)} = F + n\,a' + \frac{n \cdot n - 1}{1 \cdot 2}\,b' + \&c.$$

whence, transposing and disregarding the second differences, we have as a first approximation

$$n_1 = \frac{F_{(n)} - F}{a'}, \qquad (1)$$

and again, as a more correct result, taking account of the second differences,

$$n_2 = \frac{F_{(n)} - F}{\left(a' + \frac{n_1 - 1}{2} b'\right)}. \qquad (2)$$

The lunar distances being registered in the ephemeris for every three hours, $n_1 = \frac{t^h}{3^h}$, $n_2 = \frac{t'^h}{3^h}$; also $F_{(n)} - F = \Delta'$, the partial difference between the given lunar distance and the one which next precedes it in the Table; hence we have

$$t = 3^h \times \frac{\Delta'}{a'},$$

and in logarithms

$$\log t = \log \Delta' + P \log a',$$

and similarly

$$\log t' = \log \Delta' + P \log \left(a' - \frac{3^h - t}{3^h} \frac{1}{2} b'\right).$$

The proportional logarithm of a' (the difference between the consecutive values of the distance) is given in the ephemeris; denoting this by Q, and the difference between it and the one which next follows it by ΔQ, we have

$$\left.\begin{aligned} \log t &= \log \Delta' + Q, \\ \log t' &= \log \Delta' + \left\{ Q - \frac{3^h - t}{3^h \frac{1}{2}} \Delta Q \right\}, \end{aligned}\right\} \qquad (3)$$

in which we should employ an interpolated value of the proportional logarithm, and assume that the variation of Q is proportional to the corresponding change in a', which is very nearly true.

Or again, wholly employing proportional logarithms,

$$\left.\begin{aligned} P \log t &= P \log \Delta' - Q, \\ P \log t' &= P \log \Delta' - \left\{ Q - \frac{3^h - t}{3^h} \frac{1}{2} \Delta Q \right\}, \end{aligned}\right\} \qquad (4)$$

by which first t, and then t', might be obtained.

It is usual however in practice only to find the approximate interval t, from the above equation, and then to determine the cor-

rection $t' - t$ by aid of a subsidiary table, which is obtained in the following manner.

Subtracting the first equation in (3) from the second, we have

$$\log t' - \log t \text{ or } \log \frac{t'}{t} = -\frac{3^h - t}{3^h} \frac{1}{2} \Delta Q.$$

But by the properties of logarithms,

$$\log \frac{t'}{t} = M \frac{t' - t}{t} \text{ very nearly,}$$

$\frac{t' - t}{t}$ being necessarily a very small fraction; M being the Modulus of our system of common logarithms 0·434294.

Hence by substitution in the above

$$M \frac{t' - t}{t} = -\frac{3^h - t}{3^h} \frac{1}{2} \Delta Q,$$

whence

$$t' - t = -\frac{t \cdot (3^h - t)}{2 \times 3^h \times M} \Delta Q,$$

which gives the correction in units of an hour, or expressing it in seconds,

$$(t' - t)^s = -\frac{t^m (180 - t^m)}{2 \cdot 605764} \Delta Q$$

$$= -t^m (180 - t^m) \, [\bar{1} \cdot 584064] \, \Delta Q \qquad (5)$$

From this formula (5) the table given in the *Nautical Almanac* and other works* is computed; the arguments being the approximate interval t expressed in minutes, and the *significant* figures of the difference of the proportional logarithms, Δ Q.

It is customary, and in accordance with the precepts given in the *Nautical Almanac* (Annual Explanation) and other works, to use the difference between the two proportional logarithms standing opposite to the two distances, which include the given distance, as Δ Q; this is equivalent to the employment of the second difference, b', in equation (2), and only takes into account the second difference which follows the function F from which we set out, taking no account of the second difference obtained by considering the values of the functions which precede it; the result is that the time obtained is *over-corrected* when the proportional logarithms are *increasing*, and *under-corrected* when they are *decreasing*, the second differences having in fact been assumed as constant when they may really not be so.

* Norie, Table LIV.; Raper, Table, 57, &c.

When there is great irregularity of variation in the successive values of the lunar distances, this circumstance may produce a very sensible error in the resulting time.

This defect can however be remedied in a very simple manner by employing the formula attributed to Bessel, and using the *mean* second difference $b_0 = \frac{b' + b}{2}$ instead of b' as before, that is to say, by deducing the correction from the formula

$$F_{(n)} = F + n\,a' + \frac{n \cdot n - 1}{2}\,b_0.$$

This would be effected in using the table by employing as the argument "difference of proportional logarithms," *not* the difference between the proportional logarithms standing opposite the distances in the ephemeris which include the given distance, but the *mean* of this difference and the one which precedes it.

Thus, suppose the given distance for which the time is required fell between 9^h and midnight, we should use the *mean* difference between the proportional logarithms standing against 6^h and 9^h, and 9^h and midnight, and so on, in like cases.

Example 1. Required the Greenwich mean time corresponding to the Moon's distance from Fomalhaut, 31° 42′ 57″, on Nov. 6, 1864. (From the *Nautical Almanac*, 1864, Explanation, p. 521.)

In this case the given distance falls between the distances in the *Almanac* at IX^h. and midnight, the approximate interval is found to be $1^h\ 28^m\ 52^s$.

The "difference of prop. logs" at IX^h. and midnight is 235, from which and the "approximate interval," $1^h\ 30^m$, we get as the correction 89^s to be subtracted, and finally the G.M.T. becomes $10^h\ 27^m\ 23^s$.

A rigorous solution of the example by the formula of interpolation, using the *mean* second difference b_0, gives the G.M.T. $10^h\ 27^m\ 31^s{\cdot}5$, which differs from the *Nautical Almanac* result by $8^s{\cdot}5$, or a little more than two miles of longitude.

Again, the "diff. prop. log." at VI^h. and IX^h. is 242, and between IX^h. and midnight 285, the *mean* of which is 263. Using this as the argument of the table, we get the correction 81^s, which gives the corresponding G.M.T. $10^h\ 27^m\ 31^s$, agreeing with the above result by a rigorous computation.

Example 2. Required the Greenwich mean time corresponding to the Moon's distance from *Pollux* 18° 5′ 2″ on February 14, 1867. (From the Explanation, *Nautical Almanac*, 1867, p. 528.)

Here the approximate interval comes out $1^h\ 30^m\ 10^s$, the distance lying between XV^h. and XVIII^h., and the "difference of the prop. logs" at those times is 247, from which and the "approximate interval" $1^h\ 30^m$, the table gives the correction 78^s to be subtracted, whence finally the G.M.T. becomes $16^h\ 28^m\ 52^s$.

A rigorous solution by the formula of interpolation gives the

G.M.T. $16^h\ 29^m\ 4^s{\cdot}5$, which differs from the *Nautical Almanac* result by $12^s{\cdot}5$, which is equivalent to three miles of longitude.

Again, the "diff. prop. logs" at XV^h. and XVIII^h. is 247, while between midnight and XV^h. it is 186, hence the *mean* difference is 216. Using this as the argument of the table we get the correction 68^s, which applied to the approximate G.M.T. gives finally $16^h\ 29^m\ 2^s$ as the corrected time, which result agrees much more nearly with that obtained by a rigorous application of the formula before mentioned.

It may possibly be objected that these are extreme cases, and that the cases involved will not generally be so large; this may no doubt be true, but as the mode of rightly employing the table only involves a very simple change of procedure, there seems to be no good reason why it should not be adopted.

Haslar, March 20, 1868.

Occultations of Stars by the Moon, and Phenomena of Jupiter's Satellites, observed at the Royal Observatory, Greenwich, from January to December, 1867.

Occultations of Stars by the Moon.

Day of Obs. 1867.	Phenomena.	Moon's Limb.	Mean Solar Time. h m s	Observer.
May 6	(*a*) 117 Tauri disapp.	Dark	8 3 56·5	C.
	117 Tauri, reapp.	Bright	8 40 52·4	C.
June 14	49 Libræ, disapp.	Dark	12 31 15·7	J. C.
Nov. 6	(*b*) Aquarii, disapp.	Dark	10 28 17·7	J. C.
8	10 Ceti, disapp.	Dark	7 7 0·5	E.

(*a*) Extremely faint: the observation doubtful perhaps to a second or two.
(*b*) Cloudy. The time noted by the observer has been increased by one minute.

Phenomena of Jupiter's Satellites.

Day of Obs. 1867.	Sat.	Phenomena.	Mean Solar Time. h m s	Observer.
June 14	III.	Occult. reapp. bisection	14 27 4·4	J. C.
28	III.	Eclipse, disappearance	13 32 46·5	D.
July 9	III.	(*a*) Transit, ingress, bisec.	12 0 11·7	K.
10	IV.	Eclipse, reappearance	13 43 28·6	E.
Aug. 10	III.	Eclipse, disappearance	13 37 34·5	D.
Sept. 5	I.	Occult. disapp. first cont.	10 41 37·2	J. C.
	I.	,, ,, bisection	10 43 21·9	J. C.
	I.	,, ,, last cont.	10 45 36·6	J. C.
5	I.	Eclipse, reappearance	13 17 26·8	J. C.

Day of Obs. 1867.	Sat.	Phenomena.	Mean Solar Time. h m s	Observer.
Sept. 6	I.	Transit, ingress, bisec.	7 58 44·3	K.
	I.	,, ,, last cont.	8 1 3·9	K.
15	III.	(*b*) Occult. disapp. first cont.	7 27 1·2	J. C.
	III.	,, ,, bisection	7 30 30·6	J. C.
	III.	,, ,, last cont.	7 34 0·0	J. C.
	II.	Eclipse, reappearance	8 58 44·2	J. C.
	IV.	Occult. reapp. bisection	9 31 10·9	J. C.
	IV.	,, ,, last cont.	9 35 10·3	J. C.
	IV.	Eclipse, disappearance	10 0 11·2	J. C.
	IV.	(*c*) Eclipse, reappearance	14 23 53·2	J. C.
20	I.	Transit, ingress, first cont.	11 23 2·0	H. C.
	I.	,, ,, bisection	11 25 3·7	H. C.
	I.	,, ,, last cont.	11 27 41·3	H. C.
21	I.	Occult. disapp. bisection	8 39 38·1	E.
22	III.	Occult. disapp. first cont.	10 47 47·8	L.
	III.	,, ,, bisection	10 50 17·4	L.
	III.	,, ,, last cont.	10 52 17·0	L.
	II.	Eclipse, reappearance	11 34 49·6	L.
Oct. 1	II.	Transit, egress, bisection	7 26 49·9	P.
	II.	,, ,, last cont.	7 29 4·6	P.
10	III.	Transit, ingress, first cont.	7 45 33·0	K.
	III.	,, ,, bisection	7 49 7·4	K.
	III.	,, ,, last cont.	7 52 2·0	K.
	IV.	Transit, egress, bisection	10 4 54·8	K.
	IV.	,, ,, last cont.	10 10 3·9	K.
	III.	Transit, egress, bisection	11 26 36·4	K.
	III.	,, ,, last cont.	11 29 40·9	K.
17	II.	(*d*) Eclipse, reappearance	8 39 51·9	H. C.
28	III.	Occult. reapp. bisection	8 21 31·7	J. C.
	III.	,, ,, last cont.	8 23 31·4	J. C.
	III.	Eclipse, disappearance	9 55 56·0	J. C.
Nov. 2	II.	(*e*) Transit, egress, bisection	6 25 41·8	J. C.
	II.	,, ,, last cont.	6 28 41·3	J. C.
6	I.	Occult. disapp. bisection	8 32 14·0	J. C.
	I.	(*f*) ,, ,, last cont.	8 35 13·6	J. C.
7	I.	Transit, egress, last cont.	8 1 18·8	C.
8	I.	Eclipse, reappearance	6 38 28·4	E.
11	II.	Eclipse, reappearance	5 48 44·8	C.
23	I.	Transit, egress, first cont.	6 18 48·6	C.
	I.	,, ,, last cont.	6 20 18·3	C.
Dec. 7	I.	(*g*) Transit, ingress, first cont.	7 56 3·2	C.

(*a*) The image blurred; the observation uncertain. (*b*) The image of the lanet very tremulous and diffused. (*c*) The image very bad. A little uncertain

in consequence of the satellite reappearing close to the third satellite. (*d*) Very uncertain: appeared very near the third satellite. (*e*) The satellite faint: the planet badly defined. (*f*) The image of the planet very ill defined and tremulous. (*g*) The image of the planet very bad.

The initials D., E., C., L., J.C., K., and H.C., are those of Mr. Dunkin, Mr. Ellis, Mr. Criswick, Mr. Lynn, Mr. Carpenter, Mr. Kerschner, and Mr. H. Carpenter.

A Synoptical Catalogue of known and suspected Binary Stars. By G. F. Chambers, F.R.A.S.

About a year and a half ago, an able working astronomer whose name is by no means so well known to the Society as it should be, Mr. A. Brothers, F.R.A.S., of Manchester, presented to the Manchester Literary and Philosophical Society a very carefully prepared catalogue of known and suspected binary stars. Extending as it did, with the introduction, over twenty-seven closely-printed pages, it will readily be inferred that it was of a somewhat elaborate character; and such, indeed, was the case, for of some of the stars as many as twenty sets of measures by all the leading observers were given.

An abstract of this catalogue, kindly prepared for me at very short notice, was afterwards published in my *Descriptive Astronomy*, but as in the said abstract the places were not brought up to any uniform epoch, and as there were other defects in it which it is not worth while to particularise, I decided to take an early opportunity of computing all the places for the one epoch of 1870, and of re-writing the catalogue generally, with such textual revision as seemed necessary to bring it down to the present state of our knowledge. With this end in view, I have carefully collated all recent publications, including especially the valuable memoir of Mr. Dawes, just published,—his last legacy to us.

It does not occur to me that it is necessary to say much else by way of introduction. All the data have been selected from the latest and most trustworthy sources, and no pains have been spared to make the catalogue all that it should be.

The signs + and − in the last two columns indicate, it need hardly be said, that the position angle or the distance is increasing or diminishing as the case may be. A note of interrogation (?) denotes probability without certainty, but ± ? means that it is wholly impossible, owing to the discordances in the measures, to pronounce an opinion one way or the other.

A star (*) is prefixed to various objects of which I have been unable to discover any recent measures. It may therefore be taken that new measures of these are much wanted.

Part I. Known Binary Stars.

No.	Name of Star.	Struve's No.	R.A. 1870. h m s	Decl. 1870. ° ′	Epoch 1880 +	Mag.	Position. °	Distance. ″
1	316 B Cephei	2	0 2 11	+78 56·2	65·7	5½, 6	295·5—	[0·38—]
2	318 B Cephei	13	0 8 53	+76 10·3	63·0	6, 6½	103·5—	0·50 —
3	*n* Cassiopeæ	60	0 41 15	+57 7·8	65·7	4, 7½	125·6+	6·75 —
4	34 Andromedæ	73	0 47 59	+22 55·5	66·0	4, 6½	349·5+	1·31 +
5	*251 P. O. Piscium	..	0 52 44	+ 0 4·9	52·8	8, 9	305·1+	18·80 ± ?
6	42 Ceti	113	1 13 9	— 1 11·5	63·0	6, 7½	343·0+	1·27 + ?
7	123 P. I. Piscium	138	1 29 15	+ 6 58·8	63·1	7, 7	28·7+	1·57 ± ?
8	586 B.A.C. Piscium	186	1 49 10	+ 1 12·1	63·8	7½, 7½	85·1+	0·30 —
9	*α* Piscium	202	1 55 18	+ 2 8·1	65·7	5, 6	325·7—	3·23 —
10	γ^2 Andromedæ B.C.	..	1 55 55	+41 42·4	65·7	5, 6·3	107·0—	0·59 ± ?
11	259 B Andromedæ	228	2 5 43	+46 52·9	62·9	7, 7	286·5+	0·90 —
12	257 Σ. Persei	257	2 15 53	+60 57·7	63·1	7, 8	183·5+	[0·40—]
13	*ι* Cassiopeæ A B	262	2 18 22	+66 49·0	62·9	4½, 7	265·8—	1·92 — ?
14	*278 Σ. Cassiopeæ	278	2 26 26	+68 43·7	57·9	8, 8½	67·6—	0·40 ± ?
15	114 B Arietis	305	2 40 9	+18 48·5	63·0	7, 8	321·7—	2·52 +
16	*ε* Arietis	333	2 51 46	+20 49·2	66·6	6, 6½	199·6+	1·14 +
17	7 Tauri A B	412	3 26 44	+24 1·7	65·7	7½, 7½	240·8—	0·50 —
18	*98 P III. Eridani	..	3 30 7	+ 0 9·8	45·8	6½, 9	235·9+	6·00 +
19	49 Cephei	460	3 48 23	+80 19·8	62·9	5½, 6½	15·6+	0·70 —
20	511 Σ. Camelopardi	511	4 7 1	+58 4·2	63·6	6, 7	294·7—	..
21	230 B Tauri	535	4 16 14	+11 1·3	63·0	7, 8	342·3—	1·73 ± ?
22	2 Camelopardi	566	4 29 39	+53 12·9	63·2	5½, 8½	299·5—	1·68 ± ?
23	577 Σ. Aurigæ	577	4 33 28	+38 15·2	62·7	7½, 8	265·5—	1·62 + ?
24	932 Σ. Geminorum	932	6 26 41	+14 50·8	63·4	8, 8½	333·2 –	2·26 ± ?
25	12 Lyncis, A B	948	6 34 44	+59 34·2	66·2	6, 6½	136·5—	1·64 +
26	*α* Canis Majoris	..	6 39 25	−16 32·4	66·2	1, 10	71·3—	10·10 —
27	38 Geminorum	982	6 47 18	+13 20·6	66	5½, 8	164·7—	5·70 —
28	1037 Σ. Gemin.	1037	7 4 42	+27 26·7	63·2	7, 7½	318·1—	1·22 ± ?
29	*α* Geminorum	1110	7 26 18	+32 10·4	65·3	3, 3½	241·4—	5·68 +
30	1157 Σ. Monoc.	1157	7 48 1	— 2 27·5	63·1	8, 8¼	256·7—	1·29 —
31	85 B Lyncis	1187	8 1 20	+32 36·3	66·1	7, 7½	56·3—	1·83 +
32	*ζ* Cancri A B	1196	8 4 45	+18 2·4	66·2	6, 7	234·6—	0·40 —
33	,, A C	..	..	..	66·2	6, 7½	140·7—	5·61 —
34	*ε* Hydræ	1273	8 39 53	+ 6 53·8	66·1	4, 8½	213·8+	3·87 ± ?
35	157 B Lyncis	1338	9 12 48	+38 44·2	66·1	6½, 7	142·5+	1·71 ± ?
36	*ω* Leonis	1356	9 21 29	+ 9 37·3	66·3	6½, 7¼	32·9+	0·30 —
37	*161 P. IX. Sext.	1377	9 36 42	+ 3 13·3	56·2	8, 10	129·4—	3·11 — ?
38	*φ* Ursæ Majoris	..	9 43 14	+54 40·2	66·4	5, 5¼	45·9+	0·24 —
39	8 Sextantis	..	9 46 4	— 7 29·5	60·3	6, 6½	38·2—	0·50 ± ?
40	*γ* Leonis	1424	10 12 47	+20 30·1	66·2	2, 4	111·4+	3·17 +
41	*145 B Leonis	1426	10 13 43	+ 7 5·0	56·1	7½, 8	271·7+	0·65 ± ?

No.	Name of Star.	Struve's No.	R.A. 1870. h m s	Decl. 1870. ° ′	Epoch 1880+	Mag.	Position. °	Distance ″
42	1457 Σ. Sextantis	1457	10 31 55	+ 6 24·9	63·2	7½, 8½	309·8+	0·91 +
43	1516 Σ. Draconis	1516	11 6 28	+74 11·7	63·3	7, 7½	70·0+	4·14 +
44	ξ Ursæ Majoris	1523	11 11 15	+32 16·0	66·3	4½, 5½	86·5−	2·25 −
45	ι Leonis	1536	11 17 8	+11 14·9	65·4	4, 7½	72·1−	2·81 +
46	191 B Virginis	1647	12 24 0	+10 45·7	63·2	7½, 8	212·9+	1·39 +
47	γ Virginis	1670	12 35 5	− 0 44·2	66·3	4, 4	164·2−	4·39 +
48	1678 Σ. Com. Beren.	1678	12 38 55	+15 5·4	60·3	6½, 7¼	204·3−	31·07 −
49	35 Com. Beren.	1687	12 46 54	+21 57·2	65·3	5½, 8½	52·8+	1·31 + ?
50	42 Com. Beren.	1728	13 3 40	+18 12·9	65·5	4½, 5	193·9+	0·25 −
51	4530 B.A.C. Virg.	1757	13 27 39	+ 0 21·1	63·3	8, 9	59·0+	2·00 ± ?
52	25 Can. Venat.	1768	13 31 53	+36 57·5	65·4	6, 7	round−	.. +
53	1785 Σ. Boötis	1785	13 43 16	+27 38·0	64·9	7, 7½	192·4+	2·60 −
54	1819 Σ. Virginis	1819	14 8 48	+ 3 44·2	63·0	7½, 8	32·3−	1·29 + ?
55	1830 Σ. Boötis	1830	14 11 33	+57 16·2	60·0	8½, 9	278·2+	5·30 +
56	π Boötis	1864	14 34 36	+16 58·6	66·4	3½, 6	100·6+	5·73 −
57	1876 Σ. Libræ	1876	14 39 31	− 6 50·6	63·3	8, 8	65·9+	1·20 ± ?
58	ι Boötis	1877	14 39 18	+27 37·4	65·4	3, 7	325·5+	2·92 +
59	ξ Boötis	1888	14 45 22	+19 38·5	65·7	3½, 6½	300·8−	5·41 −
60	44 Boötis	1909	14 59 31	+48 9·7	63·3	5, 6	239·5+	4·75 +
61	1 B Cor. Borealis	1932	15 12 43	+27 18·7	63·2	6, 6½	290·2+	1·18 −
62	η Cor. Borealis	1937	15 17 50	+30 45·6	66·5	6, 6½	33·1+	1·12 +
63	μ^2 Boötis	1938	15 19 36	+37 48·2	66·5	8, 8½	180·3−	0·30 −
64	δ Serpentis	1954	15 28 36	+10 58·5	66·4	3, 5	189·8−	3·42 +
65	γ Coronæ Borealis	1967	15 37 16	+26 42·5	66·5	4, 6½	round	..
66	51 Libræ A B	1998	15 57 13	−11 0·8	66·5	4½, 5	161·0+	0·40 +
67	,, A C	..	..	..	65·5	4½, 7½	69·6−	7·10 +
68	49 Serpentis	2021	16 7 14	+13 52·8	64·8	7, 7½	324·6+	3·53 + ?
69	2026 Σ Herculis	2026	16 8 17	+ 7 42·4	65·4	8½, 9½	326·1−	1·50 −
70	σ Coronæ Bor. A B	2032	16 9 49	+34 11·4	65·8	6, 6½	192·4+	2·97 +
71	,, A C	..	..	..	62·6	6, 11	88·3−	51·04 +
72	λ Ophiuchi	2055	16 24 21	+ 2 16·3	65·5	4, 6	25·2+	1·51 +
73	ζ Herculis	2084	16 36 24	+31 50·1	66·8	3, 6	229·2−	0·83 +
74	2106 Σ. Ophiuchi	2106	16 44 58	+ 9 37·9	63·5	6, 8	321·3−	0·50 −
75	167 B Herculis	2107	16 46 42	+28 52·9	65·0	6⅓, 8⅓	189·3+	0·93 −
76	*270 P. XVI. Ophiu.	2114	16 55 44	+ 8 38·5	59·3	6⅓, 7½	147·3+	1·31 ± ?
77	210 B Herculis	2120	16 59 30	+28 16·1	66·4	6½, 9	272·9−	3·56 +
78	μ Draconis	2130	17 2 39	+54 38·7	65·6	4, 4½	181·8−	2·79 −
79	36 Ophiuchi	..	17 7 20	−26 23·9	62·4	4⅓, 6½	212·4−	4·22 −
80	δ Herculis	2137	17 9 42	+24 59·7	66·7	4, 8½	179·6+	20·18 −
81	ϱ Herculis	2161	17 19 12	+37 16·1	62·3	4, 5½	309·5+	3·61 ± ?
82	*5910 B.A.C. Ophiu.	2173	17 24 42	− 0 58·5	54·6	7, 7	150·5−	1·37 +
83	μ^1 Herculis B C	..	17 41 23	+27 48·1	66·7	10½, 10¾	85·0+	1·10 −
84	τ Ophiuchi	2262	17 56 0	− 8 10·6	66·7	5, 6	248·0+	1·60 +

No.	Name of Star.	Struve's No.	R.A. 1870. h m s	Decl. 1870. ° ′	Epoch 1880+	Mag.	Position. °	Distance. ″
85	70 Ophiuchi	2272	17 58 52	+ 2 32·5	66·6	4½, 7	101·1 —	5·27 —
86	α Lyræ	..	18 32 32	+38 39·9	65·6	1, 11	150·1 +	46·15 +
87	ε¹ (4) Lyræ	2382	18 40 0	+39 32·0	63·1	5, 6½	19·3 —	3·04 — ?
88	ε² (5) Lyræ	2383	..	..	65·8	5, 5½	142·6 —	2·35 ± ?
89	*2402 Σ. Serpentis	2402	18 43 30	+10 31·4	56·6	8, 8½	213·4 +	0·89 + ?
90	274 P.XVIII.Aquil. A B	2434	18 56 3	— 0 53·5	64·6	9, 9	136·8 —	24·29 —
91	,, B C	..	..	..	64·6	9, 10½	69·6 —	1·79 ± ?
92	2455 Σ. Vulpeculæ	2455	19 1 19	+21 58·9	64·9	7½, 9½	115·5 —	3·53 —
93	108 P. XIX. Drac.	2509	19 15 35	+62 58·3	62·9	6½, 8	343·7 —	0·80 +
94	δ Cygni	2579	19 40 54	+44 48·8	66·6	3½, 9	348·3 —	1·70 + ?
95	400 O. Σ. Cygni	..	20 5 40	+43 34·5	61·6	7½, 8½	316·7 —	0·62 — ?
96	*2696 Σ. Delphini	2696	20 27 6	+ 5 0·0	56·6	8, 8½	310·2 +	0·72 —
97	2708 Σ. Cygni	2708	20 33 45	+38 11·1	53·8	7, 9	340·5 —	16·00 +
98	λ Cygni	..	20 42 20	+36 0·8	67·0	6, 7	92·5 —	0·69 ± ?
99	*4 Aquarii	2729	20 44 32	— 6 6·7	56·8	6, 7	107·8 +	0·30 —
00	ε Equulei A B	2737	20 52 35	+ 3 47·9	66·7	5½, 7½	290·2 ± ?	1·06 +
01	,, A C	..	..	..	66·7	5½, 7½	73·1 —	10·55 — ?
02	61 Cygni	2758	21 1 4	+38 5·1	66·7	5½, 6	111·7 +	18·76 +
03	11 Arg.XXIV.Ceph.	..	21 11 4	+63 52·2	66·8	7¼, 7¼	244·5 —	0·98 +
04	20 B Pegasi	2799	21 22 58	+10 30·6	63·0	6½, 7½	317·5 —	1·44 +
05	33 P. XXII. Pegasi	2877	22 8 3	+16 33·0	63·6	6½, 9½	342·2 +	8·99 +
06	ζ Aquarii	2909	22 22 7	— 0 41·1	67·0	4, 4½	336·3 —	3·32 —
07	2934 Σ. Pegasi	2934	22 35 35	+20 45·1	63·8	7½, 9	164·7 —	1·21 ± ?
08	π Cephei A a	..	23 3 45	+74 41·1	65·7	5, 10	6·0 +	1·15 —
09	ο Cephei	3001	23 13 16	+67 24·0	62·5	6, 8½	182·1 —	2·28 — ?
10	69 P.XXIII. Aquar.	3008	23 17 2	— 9 10·4	63·0	8, 8½	262·8 —	5·59 —
11	8372 B.A.C.Cassiop.	3062	23 59 24	+57 42·3	65·7	6½, 7½	269·9 +	1·43 +

Part II. Suspected Binary Stars.

No.	Name of Star.	Struve's No.	R.A. 1870. h m s	Decl. 1870. ° ′	Epoch 1880+	Mag.	Position. °	Distance. ″
1	44 Σ. Andromedæ	44	0 31 8	+40 16·3	65·0	8½, 9	263·2 +	8·66 +
2	10 Arietis	208	1 56 16	+25 18·5	63·0	6, 8½	33·9 +	1·43 —
3	234 Σ. Cassiopeæ	234	2 7 47	+60 44·8	63·4	8, 8½	231·4 —	0·70 —
4	84 Ceti	295	2 34 33	— 1 14·8	64·0	6, 10	324·7 —	4·63 —
5	367 Σ. Ceti	367	3 7 18	+ 0 14·3	64·0	8, 8	257·1 —	0·50 —
6	η Orionis	..	5 17 56	— 2 31·1	66·9	4, 5	86·1 ± ?	0·95 ± ?
7	32 Orionis	728	5 23 49	+ 5 50·9	63·3	5, 6½	192·2 —	..
8	749 Σ. Tauri	749	5 29 6	+26 50·5	63·0	6½, 6½	186·4 —	0·60 — ?
9	*15 Monocerotis A B	950	6 33 49	+10 0·9	52·1	6¼, 9	212·3 +	3·21 +
10	14 Lyncis	963	6 40 36	+59 35·9	63·4	6, 6	59·5 +	0·70 —
11	μ Canis Majoris	997	6 50 8	—13 52·6	64·0	5, 8½	337·2 —	2·76 —
12	13 P. XIII. Cancri	1202	8 6 26	+11 14·5	63·1	8, 10	327·4 —	2·50 + ?

No.	Name of Star.	Struve's No.	R.A. 1870. h m s	Decl. 1870. ° ′	Epoch 1880+	Mag.	Position. °	Distance ″
13	1216 Σ. Hydræ	1216	8 14 42	− 1 10·8	63·3	7, 7½	151·1 +	..
14	σ^2 Ursæ Majoris	1306	8 58 55	+67 39·7	63·2	6½, 9½	253·5 −	3·25 −
15	1316 Σ. Hydræ A B	1316	9 1 23	− 6 36·4	64·8	7, 11½	138·4 −	6·74 ± ?
16	116 B Hydræ	1348	9 17 37	+ 6 54·5	63·1	7½, 7½	328·1 −	1·66 +
17	*1357 Σ. Hydræ	1357	9 21 59	− 9 25·6	56·2	7, 10½	59·5 +	7·60 + ?
18	1500 Σ. Leonis	1500	10 53 15	− 2 44·4	60·3	7½, 8	315·8 −	1·15 +
19	1781 Σ. Virginis	1781	13 39 36	+ 5 46·0	65·7	7, 8	251·7 +	1·10 +
20	238 P. XIII. Virg.	1788	13 48 9	− 7 25·1	64·8	6½, 7¼	67·7 +	2·36 ± ?
21	121 B Boötis	1825	14 10 34	+20 44·2	64·4	7, 8	178·8 −	3·89 +
22	70 P. XIV. Libræ	1837	14 17 42	−11 4·6	65·0	7, 8½	314·1 −	1·34 −
23	1863 Σ. Boötis	1863	14 33 37	+52 7·4	64·3	7, 7	95·2 −	0·77 +
24	ζ Boötis	1865	14 34 56	+14 17·2	64·8	4½, 5	303·2 −	1·02 −
25	*260 B Boötis	1867	14 35 13	+31 50·8	49·4	8, 8½	18·5 −	1·33 −
26	1883 Σ. Boötis	1883	14 42 24	+ 6 30·5	63·3	7, 7½	262·7 −	0·80 −
27	1934 Σ. Boötis	1934	15 12 45	+44 15·7	64·8	8, 8½	38·1 −	6·05 +
28	1957 Σ. Serpentis	1957	15 29 46	+13 21·0	63·5	8, 9	155·7 −	1·53 +
29	21 Ophiuchi	..	16 45 19	+ 1 25·8	65·6	6½, 8	167·6 −	1·33 ± ?
30	3107 Σ. Ophiuchi	3107	16 51 34	+ 4 7·0	64·5	8, 8½	104·3 −	1·32 −
31	α Scorpii A a	..	16 21 26	−26 8·5	66·0	1, 8	272·9 −	2·92 − ?
32	281 B Herculis	2165	17 21 11	+29 34·5	64·6	7½, 8½	51·2 +	7·10 +
33	2199 Σ. Draconis	2199	17 36 12	+55 49·7	63·0	7, 7½	101·4 −	1·65 ± ?
34	μ^2 Herculis A B	2220	17 41 23	+27 48·1	66·7	4, 10½	243·9 +	31·19 +
35	*73 Ophiuchi	2281	18 3 6	+ 3 58·1	54·6	6, 7½	252·0 −	1·32 −
36	417 B Herculis	2289	18 4 21	+16 27·0	63·0	6½, 7½	234·3 −	1·24 ± ?
37	*2384 Σ. Draconis	2384	18 38 36	+66 59·1	54·8	8, 9	332·8 +	0·35 −
38	2437 Σ. Sagittæ	2437	18 56 15	+18 58·8	63·0	7½, 7½	71·4 −	0·80 −
39	2454 Σ. Lyræ	2454	18 59 46	+30 11·8	65·3	8, 9	225·9 +	1·26 +
40	22 B Cygni	2525	19 21 30	+27 3·1	65·2	7, 7½	240·8 −	0·60 −
41	2544 Σ. Aquilæ A B	2544	19 30 53	+ 8 1·3	64·2	7, 9½	208·9 −	1·20 +
42	2556 Σ. Vulpeculæ	2556	19 33 50	+21 26·3	64·9	7, 7	167·7 −	..
43	2576 Σ. Cygni	2576	19 40 40	+33 18·4	63·3	7½, 8¾	308·8 −	3·27 −
44	*2640 Σ. Draconis	2640	20 3 4	+63 31·3	41·8	7, 11	23·2 −	4·99 +
45	2744 Σ. Aquarii	2744	20 56 27	+ 1 1·3	63·2	6, 7	177·5 −	1·50 ± ?
46	2746 Σ. Cygni	2746	20 55 22	+38 33·3	63·3	8, 9	283·7 +	0·80 − ?
47	*2760 Σ. Cygni	2760	21 1 27	+33 36·7	51·9	7, 8	223·9 ± ?	10·90 −
48	50 P. XXI. Cygni	..	21 9 21	+40 36·9	59·7	7, 7¼	126·4 −	1·02 − ?
49	29 B Pegasi	2804	21 26 57	+20 8·6	64·9	7, 8	324·5 +	2·75 −
50	*37 Pegasi	2912	22 23 24	+ 3 46·4	57·1	6, 7	117·5 +	0·74 −
51	2928 Σ. Aquarii	2928	22 32 38	−13 17·0	63·1	8, 8¼	319·3 −	4·38 −
52	219 P. XXII. Aquar. A C	2944	22 41 8	− 4 54·1	62·7	7, 8	146·6 −	50·67 −
53	2976 Σ. Piscium B C	2976	23 1 26	+ 5 54·2	57·4	9½, 10	183·2 +	16·31 +
54	3046 Σ. Ceti	3046	23 50 0	−10 12·7	63·9	8, 8¼	241·5 +	2·90 +
55	37 B Andromedæ	3050	23 52 48	+33 0·3	64·8	6. 6½	199·5 +	3·17 −

Notes to Part I.

1. 316 B Cephei. Epoch of distance = 1857·5.
12. 257 Σ. Persei. Epoch of distance = 1857·5.

51 Libræ. Perhaps the measures of A C ought rather to be put in Part II., the reality of a change not being assured. There is some confusion in the designation of this star: Brothers at the instigation of Dawes terms it ξ Scorpii, as also did Smyth, but in retaining the appellation "51 Libræ" I have followed the better supported usage.

5910 B.A.C. Ophiuchi. Secchi gives, as measures of Position angles, W. Struve, 1830, 323°; Mädler, 1843, 166°; and himself, 1858, 325°, and hints at Mädler having made a mistake of 180°.

Dawes gives Struve as above; many by himself all about 160°, and Mädler 1854, 150°.

The true explanation would seem to be that different observers had treated, some one and some the other star as A. The magnitudes of the components being nearly or quite identical, this is not matter for much surprise.

2708 Σ. Cygni. A diminution of 20° in the angle of position in thirty years, 1823–53, is clearly established; but the change in the other element is far more marked. The distance has, in like period, increased from 9″·5 to 16″·0 an amount which is very noticeable by reason of its magnitude.

Note to Part II.

2760 Σ. Cygni. In the twenty-six years, 1825–51, the position remained absolutely identical, but the distance diminished with extreme regularity from 14·3″ to 10·9″.

On some Markings seen on Venus. By John Browning, Esq.

On the afternoon of the 14th of March I had been watching some Sun-spots with Mr. Barnes' 10¼-inch silvered-glass reflector. The Sun being obscured by some trees, about 5 o'clock I set *Venus* off on the circles, and found definition better than usual. Knowing the exact direction in which to look, I now saw that the planet was visible to the naked eye. Returning to the telescope, I saw a long gradation of light from the terminator, and the extreme edge appeared unsymmetrical. Regarding the planet more attentively, I perceived a faint mottling which extended inwards from the terminator across more than one-third of the visible disk. These markings caused the planet to look something like the Moon, when it is seen faintly with a low power, in a small aperture, through a mist.

The drawing which I have now the honour to exhibit before the Society represents the appearance I have described as nearly as I have been able to portray it; but it should be viewed by oblique light, and held at some distance from the eye.

The northern horn of the planet is somewhat bulged near the termination, and then blunted just at the extremity. This I have since been able to verify.

A white spot will be noticed near the edge of the disk, opposite the terminator. This was probably caused by a cloud, such as I have often seen on *Mars;* where, when they are near the edge of the disk, they rival the ice-caps in brilliant whiteness.

On a subsequent occasion, when observing *Venus* with my own 12¼-in. silvered-glass reflector, I found the air even steadier than on the evening that I made the drawing. I had steady definition with whole aperture, and power 400; yet I could not perceive any markings on the disk. From this I conclude that the visibility of the markings depends on some peculiar condition of the planet's atmosphere.

I think I am justified in saying that *Venus* is better shown by a good reflector than by an achromatic. I do not say this only because I see it myself better; but because, at different times, I have been told by Messrs. De La Rue, Le Suer, and With, who all used reflectors, that they have made out markings on the disk of the planet, while the late lamented Mr. Dawes could never see any markings with an achromatic.

A reflector of large aperture used with a single surface reflecting eye-piece, gives, I believe, the best definition at present obtainable of this difficult object.

I propose to use such an arrangement constantly for the next few months, and trust I may be enabled to bring some more definite results before the Society.

On the Lustre of Venus. By C. H. Weston, Esq.

Sir John Herschel, in his *Outlines of Astronomy* (Art. 467), remarks that *Venus* appears occasionally in the western sky after sunset with dazzling lustre, and in favourable circumstances may be observed to cast a pretty strong shadow; and adds in a note, that such "must be thrown upon a white ground."

On the 15th inst. the sky was very clear, but subject to occasional local condensations. *Sirius*, *Orion*, and *Venus* came out with striking brilliancy; for the humid state of the atmosphere, so long as it remained transparent, necessarily produced great optical exaggeration of the heavenly bodies.

About 8 o'clock P.M. when *Venus* was in the north-westerly part of the sky, and myself and others were passing along a wall at right angles with the planet, we were struck with the shadows of all the figures projected on the wall by the vivid light which *Venus* then exhibited. I directed especial attention to the fact as one of not very usual occurrence; and the more worthy of note because the wall possessed neither whiteness nor smoothness of surface. It was built of rough stone containing much iron, which had given to it by long exposure a decidedly ferruginous brown colour, and its face was also very uneven. As the pillars of the

gateway consisted of worked Bath stone with level surfaces, I was desirous of testing more carefully the shadows thrown upon them, and here I could detect the shadow of my walking-stick, although even the worked stone was turned to a brownish white by atmospheric influence.

Of course, in the present instance, the relative position of the wall and the planet offered great advantages for the perception of shadows. Had they been at a considerable angle to each other, the delicate shadows would have been cast too far from the figures to have attracted notice, and the defining outlines of such would also have been less marked.

The wall and the gate pillars were in a clear atmosphere, on an elevation about 740 feet above the mean sea-level.

That shadows should have been cast by *Venus* in her present optical position is the more remarkable because she will not attain her greatest brilliancy (before her inferior conjunction) until 9th June.

How decided therefore (under similar favourable conditions) would these shadows have been, had they been cast by *Venus* in the greatest brilliancy of her eastern elongation upon a smooth and whitewashed wall, as suggested by Sir John Herschel.

Ensleigh Observatory, Lansdowne (above) Bath,
March 16, 1868.

Description of Zenith Telescope of U. S. Coast Survey.

By G. Davidson, Esq.

(*Extracts from two Letters to Prof. Piazzi Smyth, Astronomer Royal for Scotland.*)

When I had the pleasure of visiting you in Edinburgh last June, I promised to send you a drawing of one of our improved Zenith Telescopes, for the determination of the latitude.

I had not forgotten it, but it has been impossible to have the drawing made, on account of the great pressure of business consequent upon Dr. Bache's illness.

I inclose you a drawing that is at least effective, if not very artistic, and at the same time give a short description of the instrument and methods of observing, with the formulæ for the reduction of the observed results.

The instrument was first devised by Capt. Andrew Talcott, of the U.S. Corps of Engineers, to practically carry out the principle based upon the proposition that, when the meridian zenith distances of two stars, at their upper culmination (one being north and one south of the zenith) are equal, the colatitude is the mean of their north polar distances. It is, therefore, necessary, to avoid arc readings, that the telescope, when pointed to any zenith distance, should be capable of being revolved on a vertical axis. And as two stars could rarely be found having

the same meridional zenith distance, those are selected in pairs (N. and S.), which culminate within a few minutes of time and within twenty minutes (of arc) of zenith distance of each other, and the difference of meridional zenith distance is measured by a micrometer, and changes of verticality by a delicate level.

Many improvements have been made by officers of the Coast Survey, where its value was at once appreciated; and its latest form is that proposed by the late R. H. Fauntleroy, Esq., of the Coast Survey, whereby stars in the zenith can be observed, and transit observations also made for time with the requisite accuracy.

The drawing gives a fair idea of the instrument with the latest improvements. The telescope is usually about 45 inches focal length; object-glass $3\frac{3}{4}$ inches diameter; magnifying power used about 100, by which we obtain nearly all the stars in the B.A.C.

Stops are clamped on the azimuth circle to denote when the instrument is on the meridian N. and S. The telescope is set to the nearest minute of the apparent mean zenith distance of the two stars; the star bisected at culmination by micrometer line, and micrometer and level read. Telescope revolved 180°, and second star observed in precisely the same manner. We allow five minutes between stars, and seven minutes between pairs.

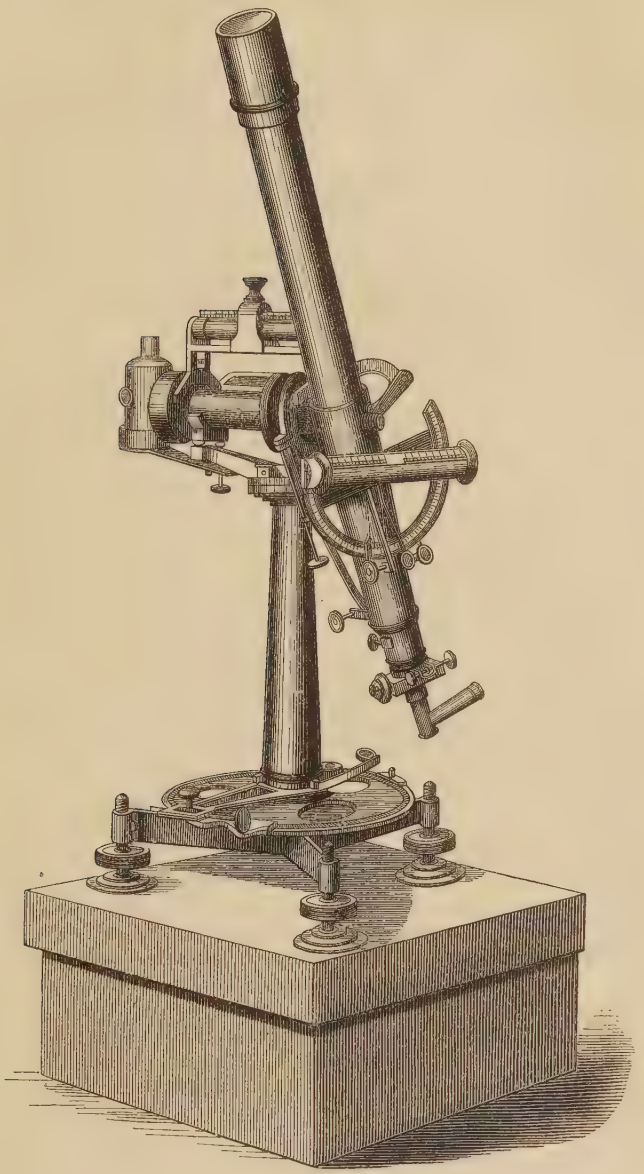
All accuracy depends upon the delicacy of the level and micrometer, and these have been particularly studied, and their values very minutely determined.

Our results show that one observation (N. and S.) is as good as the average places of the B.A.C. But I cannot enter into too much detail, and will merely refer you to a very full article on the subject in Chauvenet's *Spherical and Practical Astronomy*, pp. 1350, published here in Philadelphia.

[Here follow certain formulæ for reduction, &c. extracted by the writer from his manuscript investigations and instructions.]

Germantown, Pa., U.S.A.,
Nov. 17, 1865.

About the matter of the Zenith Telescope, you are perfectly at liberty to use anything I have written or write to you about it; and I have interested myself in the matter since receiving your note, so that I hope to receive in two or three weeks an engraving of the instrument from the groundwork of what I sent you. I have written to Prof. Benjamin Peirce, Superintendent U. S. C. S., upon the subject, and he authorises me to say that if you, or any parties, desire a Zenith Telescope constructed, he will have the work upon it superintended by a Coast Survey officer, and will specially see that all the "improvements" of the instrument, as developed in field operations, are specially attended to, and adopted. I have inquired of the maker who constructs all ours, and have from him the following reply:—



Zenith Telescope of the United States Coast Survey. Prof. A. D. Bache, Supt.

Zenith Telescope of		30-inch	focal length	850	dollars.
,,	,,	36	,,	950	,,
,,	,,	45	,,	1200	,,

He has one of 51 inches focal length, which he will sell at the price for making a 45-inch. The above rates are in the U.S. legal tender currency.

After using the 45-inch telescopes for years, I am constrained to say that their weight oppresses me on mountain duty, and I prefer one of the smaller. I will obtain for you some of the results obtained by a 30-inch. In the meantime I think a 36-inch telescope would be the best under all circumstances. The overseeing of the making of the instrument by the Coast Survey would, of course, be gratuitous.

When I send you the engraving, I will try to add a copy of all the formulæ, &c., lest I overlooked anything in my former description, which was hurriedly drawn up. In latitude observations I have no hesitation in saying, after twenty-two years' practical experience in using prime vertical transits, vertical circles, and Airy's zenith sector, that the zenith telescope of the Coast Survey stands far above them. The elaborate discussion of the results obtained by all the different methods has developed the same conclusion.

In such work as I have been engaged in upon the Alaska Coast, and expect still further to carry forward, I am evolving an instrument to combine the properties of the portable transit and zenith telescope. I inclose you an engraving of the Coast Survey portable transit, with telescope of 36 inches focus. The broad idea covering my proposed instrument is to have a double horizontal frame, or one beneath that of the transit; one revolving upon the other with a short central vertical hollow axis. When to be used as a transit, the two horizontal frames are securely clamped together: when to be used as a zenith telescope, these screws are unclamped, and the instrument will revolve in azimuth, with stops on each side, to which the upper part is brought, to make the telescope play in, or very near, the plane of the meridian. This instrument will also be very useful in determining the azimuth of a mark by circum-elongation of a close polar star. This is necessary in our geographical reconnaissance of a coast where bad weather may close upon the observer when changing his transit and setting up a zenith telescope. I put my transit in the meridian generally in less than 1½ hours; observed eight or ten transits for time and instrumental corrections; then obtain ten pairs of zenith telescope stars for latitude, which usually has a probable error of less than 0″·2. The instruments are generally removed by one or two o'clock A.M., and the vessel at another station 50 or 60 miles off during the day, and ready for another set of observations at night.

Where the latitude is required for a main triangulation station, the zenith telescope observations embrace about five ob-

servations upon as many different nights (*i.e.* one observation per night) upon thirty pairs of stars. Of course this supposes that the places of the stars are well determined. We use the B.A.C. as a field basis, and after the stars are selected for work, the corrections to their B.A.C. places are used in the computations.

Germantown, Pa., U.S.A.,
February 11, 1868.

Observations of the Lunar Crater Linné.

By the Rev. T. W. Webb.

The very unfavourable weather which we have had for a long time has prevented me from turning to much account the very superior defining power of the $9\frac{1}{3}$-inch silvered speculum by Mr. With, now in my possession; but I have had a few views of the spot *Linné*, in addition to one which was made known at an early period, and the most satisfactory of these I beg permission to communicate to the Society, in the hope that they may be received as a slight addition to the evidence which is being accumulated as to the nature of this mysterious object.

1867, Nov. 17^d 18^h 30^m. Power about 170. Definition very fine, though a little fluttering. At, or very near, *Linné*, a minute, white, not especially brilliant speck stands exactly on a dark grey terminator, quite insulated, and not surrounded by any white cloud. It is impossible to say whether it is *Linné*, but from comparison with Lohrmann I believe it to be so.

1867, Dec. 16^d $18^h 45^m$. Power 170. Air very fluttering and often clouded. Terminator through W. wall of *Maurolycus* and *Aristoteles;* a very little beyond *Bessel.*

Linné an inconspicuous object; the white cloud has become very faint, and is ill bounded; at its E. edge is a small speck of greater brightness, apparently a minute hill; but definition is too unsteady.

The preceding observations were in waning illumination, as the dates will show. In the next it was enlightened the opposite way.

1868, Jan. 1^d 7^h 30^m. Power about 212. Air very steady, but constant clouds of varying thickness. Terminator through the pass between *Caucasus* and the *Apennines;* the E. edge of the *Mare Serenitatis* next *Apennines* is, however, still in shade.

Linné. A very hasty view seemed to show a crater of several seconds in diameter, with a shallow white interior, and a very narrow shadow on the W. side; but the cloud became too thick before it could be verified. It may have been about the size represented by Lohrmann.

The next observation was two days only before full.

1868, Jan. 7^d 9^h 30^m. Power 170. Sharp, but fluttering, and

very frequently clouded. Terminator bisecting *Seleucus*, and half its own breadth beyond *Phocylides*.

Linné neither so large nor so bright as *Posidonius* γ. Each, but most certainly *Linné*, seems to have a minute brighter spot in the centre.

The next two were in the wane.

1868, Jan. 14^d 19^h 15^m. Power 170. Sharp and clear, but often obscured by cloud. Terminator through W. edge of *Cyrillus* and *Catharina*, and the Serpentine Ridge of *M. Serenitatis*.

Linné. A white spot, not very large or bright, with something brighter in its centre, not very readily seen. It was a little E. of the centre, and looked, as far as I could judge, like a very small white hill.

1868, Jan. 15^d 19^h 20^m. Power 110. Generally thick haze; often cloud; bad definition in a few clear intervals. Terminator about two-thirds diameter of *Autolycus* beyond *Linné*.

Linné. I was for a long time deceived by mistaking for this object *Linné* B, which was a little beyond the terminator; and it was not till I had examined the map that I perceived my error: it had then become cloudy, and had it continued so, I should have believed that nothing had been visible on the site of *Linné* except some grey mounds; but fortunately a clear interval of a few minutes enabled me to repeat the observation; when I found in the right position a very small white spot, very much smaller than the white cloud, of which there was not a trace; though distinctly defined upon the dark grey surrounding level, it was by no means a bright object. It was, I think, elongated N. and S., and had the aspect of a hill, but perhaps more from prepossession than any real evidence, for under present definition it was impossible to be certain of any shadow on the W. side. A winding grey ridge passed from S. to N. between it and the terminator, which it touched N.W. of *Linné*, and then returned in a less connected form towards E. The white hill was so far from it, that I question whether the cloudy patch, if visible, would do more than reach the foot of the ridge.

The last observation was in increasing illumination, 2^d after first quarter.

1868, Feb. 3^d 7^h 15^m. Power 212. Sharp, but unsteady definition. Terminator two-thirds across *Sinus Iridum*, of which the whole border is enlightened.

Linné is decidedly less bright than *Posidonius* γ. I sometimes fancied I saw a trace of the shadow of the W. side of a low ring, as large as that drawn in the map; but my impression was faint, and while writing subsequently I feel less certain about it than I could wish.

10^h 30^m to 11^h 15^m. Definition has become unusually beautiful, though not perfectly steady, and clouds frequently interrupt the observation. *Sinus Iridum* all enlightened, but E. part in heavy penumbra. *Linné* at best moments, — in fact usually, is very distinctly seen as a minute deep crater with black

interior shadow and bright ring: once or twice I even fancied I could see part of the bottom in bright illumination; but of this I could not be certain. It lies in the centre of the white cloud, of which it occupies $\frac{1}{3}$ or $\frac{1}{4}$. I thought it bore about the same proportion to the neighbouring crater *Linné* A, but I did not at the time make a direct comparison of the cloud and this crater. It was, I think, about the size of the minute crater *n. f.* the N. end of the *straight wall*, and could not have exceeded,—probably did not attain,—1″ in interior diameter. It was seen, but not so well, with a power of 450; the eye-piece, however, was not quite equal to the other (a microscopic doublet of admirable sharpness), and the air might have changed.

This last observation is sufficiently remarkable, from the evidence afforded by the distinct black shadow, of the extraordinary depth and steepness of the crater, above which the sun could not have been elevated much less than 50°.

Hardwick Parsonage, Feb. 8, 1868.

The Lunar Crater Linné. By Edward Crossley, Esq.

On Monday evening, March 30th, 1868, I had an excellent opportunity of viewing *Linné*. Age 6·6 days; aperture $9\frac{1}{3}$ refractor. Equatoreal by Cooke.

With low powers the shadow of the central eminence was easily seen. With 240 it was plainly resolved into a small crater, and with 470 the crater was brought out into as strong relief as any of the other craters on the *Mare Serenitatis*. It appears to be about a mile in diameter.

The obscuration round the crater is very much subdued to what it was last year, and appears circular, and somewhat dished, but badly defined at the edges.

In October 1867, the Moon waning, with the terminator on Bessel, and with the same power and aperture, I could only make out the shadow of an eminence in the centre of *Linné*.

On the Tuesday evening, March 31st, 1868, though the Moon was a day older, the central crater was quite an easy object.

Halifax, April 1st, 1868.

The Lunar Crater Linné. By Capt. Noble.

The night of Monday, the 30th of March ult., being peculiarly fine, I occupied myself from $8^h\ 20^m$ to 9^h L.M.T. in the examination of *Linnæus;* employing powers of 154 and 255 in my 4·2-inch Equatoreal for that purpose.

The detail was even better and more sharply seen than on the 3d of last November, and the outline of the wall of the crater

was exceedingly plain. The west and south-west portions of this wall cast a distinct shadow in the interior: no such shadow, however, was projected by the east wall on the surrounding *Mare*. Hence it is evident that, shallow as is the western half of the ring, the eastern is shallower still. I could see nothing of the minute crater. The Moon's age at the mean epoch of this observation was 6·56 days.

At $11^h\ 10^m$ L.M.T. I returned to my scrutiny of *Linnæus*. The shadow of the western wall had disappeared, and it was only by sustained attention that *Linnæus* could be made out as a crater at all. A cursory glance showed it as a nebulous spot. The Moon set about 20 minutes after midnight, but I am persuaded from what I saw, that very shortly afterwards nothing but the well-known hazy, ill-defined patch can have occupied the place of *Linnæus*, in every locality where the Moon was above the horizon.

Forest Lodge, Maresfield, Sussex,
8th April, 1868.

Proposal for a new Star Atlas.

By R. A. Proctor, B.A.

It appears to me that a series of star-maps fulfilling the following conditions would form a desirable addition to scientific libraries:—

The series should consist of

I. A moderate number of maps,
II. Not too large for convenient use,
III. Uniform in size and shape,
IV. On the scale of an 18-inch globe (at least),
V. With little distortion, and
VI. With little variation of scale or area,
VII. On a *central* projection or construction uniform for each map; and
VIII. Exhibiting satisfactorily the connection between the different maps of the series.

A little consideration will show that all these conditions are important, and some of them essential for charts intended to occupy a space midway between popular maps and such charts as Argelander's. Accordingly it would not suffice merely to increase the scale of any extant popular maps. The effect of such a process is seen in the large maps of the Society for the Diffusion of Useful Knowledge, which are not only too unwieldy for convenient use, but represent small star-groups with preposterous distortion. In fact, the gnomonic projection, which has valuable properties for popular maps, seems wholly unsuited for maps of a higher class. The astronomer seeking to master the configura-

tion of small star-groups, the depths within which he proposes to explore, requires more efficient aids than "straight-line pointing," which may co-exist (as every mathematician knows) with the most egregious distortion and scale-variation.

The stereographic projection is far superior to the gnomonic for such purposes as we are now considering. But there is a somewhat rapid scale-variation in this projection, which is likely to prove misleading. It will be found difficult to propose any plan for stereographic maps in which the variation of scale and area would be nearly so small as in the construction I am about to propose.

My plan is to divide the sphere into twelve spherical pentagons, the centres of which would be the angular points of a dodecahedron inscribed within the sphere. The poles of the sphere should be the centres of two of the pentagons.

It will be observed that this mode of dividing the sphere is that adopted in my gnomonic star-maps. But, instead of the gnomonic projection I propose the use of the equi-distant construction; and I suggest, also, that the small circles surrounding each spherical pentagon should be made the boundaries of the twelve maps.

The scale-variation in the above arrangement would be exceedingly small: it would clearly be represented by the ratio of the *spherical* radius to the *true* radius of the small circle bounding each map; *i.e.* by the ratio

$$\sin 37^\circ\ 22'\ 38''{\cdot}5 : \text{arc } 37^\circ\ 22'\ 38''{\cdot}5$$

or

$$40 : 43$$

very nearly. This ratio would also represent the variation of area.

I send with this the half of a polar and the half of an equatorial map, on the scale of an 18-inch globe, divided to every degree of arc. The equatorial map was drawn as follows: the position of the point of intersection of every fifth circle of right ascension with every fifth declination-parallel was calculated by the usual spherical formulæ, and marked down, accordingly, in the map.

Of the ten equatorial maps, only two would have their *dark* circles of R.A. in the position shown in the half-map. In all other respects the lines of R.A. and declination would be alike in the ten equatorial maps.

Maps on this scale might contain all the 8377 stars of the B.A. catalogue, and at least 2000 nebulæ. I would suggest the reduction of star-places to the year 1880 as in my star-maps. Perhaps, also, the introduction of the heliocentric orbits of the planets would make the series more useful.

The connexion of the maps would be sufficiently indicated by the overlapping segments; or, my gnomonic maps might be used as index-plates.

The boundaries of the constellations would have to be introduced, but I think the constellation-figures should be omitted. They would disfigure a scientific atlas. If my small maps were used as suggested, all that would be required in this way would be supplied.

The addition of duplicate maps showing the stars only on a black back-ground would, I think, be desirable.

The longitude and latitude lines would not be required, as every observer is, doubtless, familiar with the processes for reducing R.A. and declination to longitude and latitude, and *vice versâ*.

I would suggest also the addition of a short line at the intersections of every fifth meridian and parallel,— such line indicating, by its length and direction, the amount and direction of the precessional change for 100 years (say) at that point of the celestial sphere.

The labour of drawing the meridians for each map could be saved by instructing the engraver to draw the meridians and parallels of one polar and one equatorial map ; and to take a sufficient number of impressions of these *on dry paper* (to avoid shrinkage). On these impressions the stars and constellation-boundaries could then be drawn in by whoever undertook the construction of the maps.

The reduction of star-places to 1880 (to the degree of accuracy required) would not take long. In my *Handbook of the Stars* there are the reduced places of 1500 stars extracted from the B. A. Catalogue. It may be noticed that the reduction is simplified when it is made for thirty years, since we have only to halve the correction in *seconds* and estimate the result as *minutes*.

The *Annals of the Astronomical Observatory of Harvard College*, published up to this date, are —

Vol. I. Part I. History and Description of the Observatory;
Vol. I. Part II. Zone Catalogue of 5500 Stars ;
Vol. II. Part I. Observations of the Planet *Saturn;*
Vol. III. Account of the Great Comet of 1858 ;
Vol. IV. Part I. Catalogue of Polar and Clock Stars. There is now published, 1867 ;
Vol. II. Part II. containing the Second Series of Zone Observations, of which the first series is continued ;
Vol. II. Parts II. III. and IV. are yet unpublished ; and
Vol. V. entitled Observations upon the Great Nebula of *Orion* by the late G. P. Bond, Director of the Observatory of Harvard College, edited by Truman Henry Safford, Director of the Dearborn Observatory of Chicago, Introd. pp. viii. to xxvi., text pp. 1 to 189.

The observations given in the volume were commenced by Prof. G. P. Bond in the year 1857. During that winter and the

next spring they were continued so far that most of the stars between the limits of the R.A. of the catalogue contained in the work, and within 20′ of Decl. of the trapezium were already observed. Owing, however, to the apparition of Donati's Comet (and also partly to the decease of Prof. W. C. Bond in 1859), the work of reduction was discontinued, and suffered an interruption of some years. But as soon as vol. iii. was finished, Prof. G. P. Bond returned to the nebula of *Orion*, and from that time until his death in 1865, devoted much of his time and of his gradually failing strength to that object.

The work was left unfinished at the author's death, and the completion as far as possible, and publication, were left to the editor. It was, therefore, necessary for him to complete those portions which were so far advanced that the form which Prof. Bond desired to give them could be recognised, and for the remainder to give as far as possible the original observations as they stood in the records of the Observatory, with the amount of reduction necessary for astronomers to derive as much advantage as possible from them.

The charts and engraving executed by Mr. Watts under the author's personal supervision are appended to the engraving of the nebula not completed in 1864, and the edition of it printed.

A drawing by Prof. W. C. Bond, from which an engraving was made for vol. iii. new series, *Mem. American Academy*, was given to Mr. Watts to re-engrave, as the original engraving did not appear sufficiently accurate, and the differences between the two are of some importance in respect to controverted points. It was, however, found an extremely difficult task to reproduce the original drawing.

Elements of Minor Planets (94) *and* (95).

The following elements calculated by Dr. Tietjen are given *Ast. Nach.* No. 1863.

(94).

1867, Nov. 28·0, Berlin M.T.

M	= 340° 30′ 39″·5	Mean Eq. 1867·0
ω	= 40 50 23·2	
Ω	= 4 32 9·3	
i	= 8 5 27·0	
φ	= 5 10 18·6	
μ	= 630″·5129	
log a	= 0·500208	

(95).

1868, Feb. 12·0, Berlin M.T.

M	= 39° 23′ 39″·3	Mean Eq. 1868·0
ω	= 144 33 46·1	
Ω	= 244 20 50·3	
i	= 12 52 6·5	
φ	= 8 32 13·3	
μ	= 659″·8598	
log a	= 0·487037	

ERRATUM IN LAST NOTICE.

Page 154, *for* H. C. Key, Esq. *read* the Rev. H. Cooper Key.

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